

Response to Eisenstein and Bunn's Null Hypothesis Comment on Cosmological Birefringence

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Eisenstein and Bunn have widely circulated a Comment (astro-ph/9704247) suggesting a non-standard null hypothesis in a test for cosmological birefringence. The Comment misrepresents the procedure used by Nodland and Ralston (B. Nodland and J. P. Ralston, *Phys. Rev. Lett.* **78**, 3043 (1997); astro-ph/9704196) and lacks statistical basis; no calculations were reported, but sweeping conclusions were drawn from eyeballing a single scatter plot. The results of the suggested procedure range from an underestimate of the statistical significance of well-correlated data, to a failure of detecting a perfect correlation in the limit of strong cuts. We verified these faults by performing the actual calculation which EB suggested, but neglected to carry out themselves. Furthermore, the calculation showed that the original correlation remains statistically significant, with the lego-plot of $1/P$ versus \vec{s} showing a persistent bump, and the anisotropy direction \vec{s} remaining in the same direction as previously reported.

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Eisenstein and Bunn's Comment [1] is weak in making sweeping judgments without much foundation, and with absolutely no calculation. It is unfortunate that the Comment misrepresents what we reported in [2], and we find it flat wrong in several assertions. We welcome the opportunity to clear up the matter.

Our angle β is not so simply defined as “the angle between the polarization direction and the galaxy axis,” but is a four-part formula involving the fundamental variables χ and ψ , the galaxy position, and the trial axis \vec{s} . In contrast to variables used in other studies, the angle β retains information about the sense of rotation being tested. We did not pick “the angle β from a uniform distribution of allowed angles.” Instead, and as reported in our paper, we picked the χ 's and ψ 's from a uniform distribution. The distributions of χ 's and ψ 's found in the data *are* quite uniform, so our Monte Carlo reproduces what the data shows. We then fed the random χ 's and ψ 's into the same formula that evaluated β for the data. The result is that the β distribution in each quadrant depends on everything, including the anisotropy of the galaxy distribution and the trial \vec{s} . The distribution of random β 's is whatever is chosen by the procedure 1 or procedure 2 Monte Carlos, and is not what Eisenstein and Bunn (EB) describe.

In the fifth paragraph in [1], EB make a claim that is unsupportable: “if the underlying galaxy distribution truly had a uniform intrinsic distribution in β , it would be impossible to measure the proposed birefringence at all.” This is incorrect, as every measurement that obtains a set of data points lying along the line $y = mx + b$ can establish a correlation between y and x , whatever the distribution of data sampled on the y -axis. For example, if the β versus $r \cos \gamma$ plane had points equally spaced and exactly along the diagonal line, the distribution over β would be uniform, but with β and $r \cos \gamma$ obviously correlated.

We think that what EB is trying to say is in the next paragraph, where “estimating by eye” as they put it, the data in Fig. 1(d) in [2] are observed to be “more tightly correlated than data uniformly distributed between 0 and $\pm\pi$.” This is put forth as something bad, or suspicious, without recognizing that there is a very good reason for the β values to be distributed this way. The figure shows data surviving the cut $z \geq 0.3$, which leaves a hole on both sides of the origin on the $r \cos \gamma$ axis. This was described in our paper. The data is correlated like $\beta = r \cos \gamma$, and naturally the β values are going to have a hole at the origin of the β axis! What about the outer ends? We run out of data at large z , so $r \cos \gamma$ has a maximum absolute value. Again, because the data is correlated, one indeed finds a shortage of β 's of large magnitude. Isn't it true that whenever there is data on the “ y -axis” well correlated with data on the “ x -axis,” one will find that the “ y -axis” distribution reflects the cuts and limits found on the “ x -axis”?

EB thus make a wrong deduction about the underlying distribution of β values. To account for their own conclusion, EB suggest for their own faux null hypothesis to “draw the angles (β) from the observed distribution” and make an estimate of the significance relative to that. This has the sound of something fancy, but what is the basis for it, and what does it really mean? Suppose one has laboratory data that exhibits a linear relation of the form $y = mx$, plotted on a diagonal line. With x -cuts like ours, the data is restricted to boxes in two quadrants. If one uses that data

and shuffles it, one makes a set of faux random data coming uniquely from the small boxes. This “random data” is far from random; it is highly pre-correlated. Comparing the correlations from random shufflings with the better correlated real data, one will find a correlation, but the baseline for what is declared “relatively likely” then gets moved up. The generic results are that the data’s correlation can be artificially made to look more probable. For example: the significance of even a (perfectly correlated) $\delta(y - x)$ distribution, when compared with such well-correlated faux randoms, would be meaningless in the limit of strong cuts, with a probability of unity for the faux randoms to be as correlated as the true x and y .

It seems to us that EB did not understand our procedure, and created a hasty argument based on a false premise from eyeballing a single scatter plot. Their opinion that we overestimated the statistical significance of our results is incorrect. The description could also mislead a reader who might not know that we did not quote any “absolute” probabilities based on statistics books, but restricted ourselves to reporting just what we found: the P-values for clearly defined and objective Monte Carlo procedures that were done two separate ways. Anyone who wants to do the work can go ahead and make a serious calculation based on some other criterion any time.

To back up our claims, we checked our belief that the EB procedure will artificially raise the baseline of a good correlation. Repeating our calculation with shuffling that kept the β distribution invariant, we found that the P-values were increased, with the smallest P-values (those forming the peak in the $1/P$ plot in Fig. 1(b) in [2]) increased by a factor of about ten above our previously reported values. Even with this change (which we don’t accept as meaningful), the correlation still looks significant. The lego-plot showing a bump in $1/P$ versus \vec{s} [our Fig. 1(b)] still showed a bump with the “EB procedure,” and the bump occurred for the same direction of \vec{s} as before. We went ahead and did the calculation, because we think it would be bad science to make general remarks in the absence of attention to the facts.

In a final complaint, EB object to the use of the scale Λ_s , which they claimed to be inappropriate because it may be non-zero for random data. The facts are, we did not use Λ_s as a statistic, but used the correlation coefficients R . EB might have considered the fact that every curve-fitting or statistical Monte Carlo procedure can generate a non-zero parameter for random data. The non-zero parameters are ignored when the correlation is not significant. In our analysis, the Monte Carlo test rejected the null hypothesis as a consequence of P-values being of order 10^{-3} . When the test rejects the null hypothesis, calculation of Λ_s is a meaningful thing to do, just as we reported.

Sweeping judgments can be dangerously wrong, and, when widely distributed without responsibility, can harm good work honestly reported.

[1] D. J. Eisenstein and E. F. Bunn, astro-ph/9704247.

[2] B. Nodland and J. P. Ralston, *Phys. Rev. Lett.* **78**, 3043 (1997); astro-ph/9704196.